1.0 INTRODUCTION

1.1 HOW TO READ THIS DOCUMENT

This document is not a summary of the facts from the vast literature on the possible health effects of extremely low frequency (ELF) electric and magnetic fields. There have been many such reviews, including some very recent ones (NAS et al., 1997), (Portier & Wolfe, 1998). Therefore, the descriptions reported in the Working Group Report published by the National Institutes of Environmental Health Sciences (NIEHS) will not be reiterated. It is available in print and on the web, although studies published since the deadline for inclusion in the NIEHS document will be described. In reaching the herewithin conclusions, however, the three reviewers will consider all studies.

In preparation for this evaluation, the California Electric and Magnetic Fields (EMF) Program held a two-day epidemiology workshop to discuss some of the most relevant epidemiological findings and methodological issues. The proceedings of that workshop, which were pivotal to some of the conclusions reported here, were published in a peer-reviewed Supplement (5) of the journal *Bioelectromagnetics* on January 22, 2001. Those who had assisted in the drafting of the 1999 NIEHS document were asked to provide updated versions of their contributions to help the reviewers in preparation of brief tabular summaries of the evidence for this document. The reader will find that chapters 1, 2, 3, and 7 cover in somewhat more detail areas covered in the Overview and Rationale of Conclusions. The latter was meant to be a brief summary of the entire document. The other chapters go into detailed discussions of the various streams of evidence and particular disease endpoints.

1.2 WHAT IS NEW IN THIS EVALUATION

NEW EVIDENCE

There have been many adequate reviews, including some very recent ones (NAS et al., 1997); (Portier & Wolfe, 1998); (IARC, 2001). The NIEHS review, in particular, was regarded as the starting point for this evaluation. Their NIEHS Working Group carried out their evaluation in June 1998. Several important studies have been published between the conclusion of the NIEHS Working Group review and this evaluation, including three major studies on childhood leukemia (Green et al., 1999b), (Green et al., 1999a), (McBride et al., 1999), (UKCSS, 1999). The deadline for including studies in this evaluation was June 24, 2000. This is later than the deadline originally mentioned in the Risk Evaluation Guidelines (REGs). Since the Department of Health Services evaluation began later than initially envisaged, the reviewers felt that it was unwise to disregard recently published, and possibly important, studies simply to observe a previously set but otherwise arbitrary date. Only one large study (van Wijngaarden et al., 2000) that dealt with suicide emerged during this extended deadline period.

In addition, the reviewers considered studies sponsored by the California EMF Program (Li et al., 2002), (Lee et al., 2002) and in the Epidemiology Workshop satisfying the criteria for inclusion in this evaluation, as specified in the Guidelines. In this final draft the DHS scientists also discuss articles that were brought to their attention during the public comment period (see Appendix 6 for additional references considered).

The document has features that were not present in the NIEHS document. One of these—presenting a graded degree of certainty of causality—is described below. Also discussed are the aspects that make up the EMF mixture that characterizes the exposure of persons who come near the power grid, the internal wiring of houses, and common household appliances. These are described in Chapter 3. The reviewers stress the notion of "mixture" because different aspects of EMF exposure (e.g., 60-cycle magnetic fields and high frequency transients) would require different actions for abatement. For each of the diseases considered, there are explicit discussions about whether the epidemiological associations observed, if real, would convey a risk from lifetime exposure that would be of regulatory interest. This is a parameter of interest to the social justice policy framework, which focuses on the individual risks of the most highly exposed. In Chapter 21 at 21.5, the baseline mortality for conditions considered possibly associated with EMFs are discussed. The reviewers ask if the attributable burden of mortality from even a very small fraction of that baseline would be of regulatory interest when compared to the mortality burden thought to be avoided by regulation of other agents. The attributable burdens of mortality or morbidity are parameters of interest to the utilitarian policy framework, which aims at the most good for the most people at the least cost. The document also attends to any evidence suggesting inequitable exposure or vulnerability to EMFs. This is relevant to the environmental justice policy framework, which is concerned with unfair distributions of risk.

Each health condition considered had at least two epidemiological studies in which there was a statistical association with some surrogate for EMF exposure. The list of conditions is similar to that discussed in the NIEHS document and includes:

- Adult and childhood leukemia
- Adult and childhood brain cancer
- Male and female breast cancer
- EMF as a “broad spectrum” carcinogen for all cancers
- Miscarriage
- Other reproductive and developmental conditions
- Amyotrophic lateral sclerosis (Lou Gehrig’s Disease)
- Alzheimer’s disease
- Acute myocardial infarction
- Suicide
- Other adverse non-cancer health outcomes (depression, electrical sensitivity)

1.3 QUALITATIVE BAYES OR DEGREE OF CERTAINTY APPROACH TO EVALUATION

The DHS scientists found the usual process of describing the pattern of evidence in some detail and then expressing an opinion (without explaining the rationale for that opinion) to be insufficiently transparent. Accordingly, they supplement the usual International Agency for Research into Cancer (IARC) procedure with an additional form of presentation and an additional form of judging whether EMFs are a cause of disease. The following table shows the questions that were systematically addressed. For definitions of epidemiological terms in the table see pages 20-22 (Sections 12.1.1 -12.1.3).

TABLE 1.1 QUESTIONS RELEVANT TO DEVELOPING A DEGREE OF CERTAINTY ABOUT CAUSALITY

EXPLANATIONS OF A STATISTICAL ASSOCIATION OTHER THAN A CAUSAL ONE
Chance: How likely is it that the combined association from all the studies of EMF and disease is due to chance alone?
Bias: How convinced are the reviewers that EMFs rather than a study flaw that can be ***specified and demonstrated*** *caused this evidentiary pattern? If no specified and demonstrated bias explains it, how convinced are they that EMFs caused these associations rather than* ***unspecified*** *flaws?*
Confounding: How convinced are the reviewers that these disease associations are due to EMFs rather than to another ***specified and demonstrated*** *risk factor associated with EMF exposure? If not due to a specified risk factor, how convinced are they that they are due to EMFs rather than to* ***unspecified*** *risk factors?*
Combined effect: How convinced are the reviewers that these disease associations are due to EMFs rather than to a combined effect of chance and specified or ***unspecified*** *sources of bias and confounders?*
ATTRIBUTES SIMILAR TO HILL'S (HILL, 1965) THAT ARE SOMETIMES USED BY EPIDEMIOLOGISTS TO EVALUATE THE CREDIBILITY OF A HYPOTHESIS WHEN NO DIRECT EVIDENCE OF CONFOUNDING OR BIAS EXISTS
Strength of association: How likely is it that the meta-analytic association is strong enough to be causal rather than due to unspecified minor study flaws or confounders?
Consistency: Do most of the studies suggest some added risk from EMFs? How likely is it that the proportion of studies with risk ratios above or below 1.0 arose from chance alone?
Homogeneity: If a large proportion of the studies have risk ratios that are either above or below 1.0, is their magnitude similar (homogeneous) or is the size of the observed effect quite variable (heterogeneous)?
Dose response: How clear is it that disease risk increases steadily with dose? What would be expected under causality? Under chance, bias, or confounding?
Coherence/Visibility: How coherent is the story told by the pattern of associations within studies? If a surrogate measure shows an association, does a better measurement strengthen that association? Is the association stronger in groups where it is predicted? What would be expected under causality? Under chance, bias, or confounding? How convinced are the reviewers that the magnitude of epidemiological results is consistent with temporal or geographic trends?
Experimental evidence: How convincing are the experimental pathology studies supporting the epidemiological evidence? What would be expected under causality, bias, chance, or confounding?
Plausibility: How convincing is the mechanistic research on plausible biological mechanisms leading from exposure to this disease? What would be expected under causality, chance, bias, or confounding? How influential are other experimental studies (both in vivo and in vitro) that speak to the ability of EMFs to produce effects at low dose?
Analogy: How good an analogy can the reviewers find with similar agents that have been shown to lead to similar diseases? What would be expected under causality, chance, bias, or confounding?
Temporality: How convinced are the reviewers that EMF exposure precedes onset of disease and that disease status did not lead to a change in exposure?
Specificity and other disease associations: How predominantly are EMFs associated with one disease or subtypes of several diseases? What would the reviewers expect under causality, chance, bias, or confounding? How much is their confidence in EMF causality for disease X influenced by their confidence that EMFs cause disease Y?

As a heuristic device, and following Huticinson and Lane (Hutchinson & Lane, 1980), the REGs suggested that these questions about the pattern of evidence be posed so that one could say the pattern is more likely under the hypothesis that EMFs contributed to the cause of that health condition or more likely under the hypothesis that chance, bias, or confounding produced the pattern. This allows the reviewers to provide the reader a rationale for the relative weight given mechanistic, animal pathology, and epidemiological evidence, and to understand which parts of the evidence suggest causality and which speak against causality.

The DHS reviewers coined the term "Qualitative Bayes Approach" to characterize a form of verbally justifying judgments about hazard that paid attention to the insights of Thomas Bayes, an 18th-century mathematician. His insights would suggest starting with some initial degree of certainty that any given agent is capable of being harmful based on knowledge about agents in general. Evidence is then accumulated on this specific agent and this changes the degree of suspicion or certainty.

Imagine a prehistoric hunter deciding whether to try some jungle fruit he has never seen before. He has an initial degree of suspicion high enough that he does not partake right away. He takes some fruit home and feeds it successively to several types of captured birds. As each species seems to survive, it seems less and less likely that the fruit would be harmful to humans. But since the leaves of the tree bearing that fruit resemble those from a tree that bears a poisonous fruit (causing the initial suspicion to be very high) the hunter's specific experiments might still leave him fairly suspicious and lead him to cruelly feed the fruit to a captive from another tribe. Only if the captive survived would his initial suspicions be allayed. This example illustrates Thomas Bayes's two key insights: As evidence builds we update our degree of certainty of harm, but at any point in time, that updated degree of certainty also depends on how suspicious we were initially. This idea is expressed mathematically by a simple formula.

Initial Odds * Relative Likelihood of Evidence = Updated Odds

The first term of the Bayes formula is the prior odds, that is, the odds that a given hypothesis is thought to merit *a priori*, before examining the evidence. In this document it is called the "prior" because it is not based on subsequent research.

The second term, the "relative likelihood," is a multiplier, calculated (or, in this case, qualitatively discussed) after scientific evidence has been collected and evaluated. The term "relative likelihood" is most properly restricted to the case where one compares the statistical likelihood of a result under one specific hypothesis relative to that under another hypothesis, usually the null. It expresses the likelihood of the observed pattern of evidence if EMFs do indeed cause disease, divided by the likelihood of that pattern if EMFs do not cause disease. The third term, the posterior, is the product of the first two and represents the odds of the risk being true after the prior has been modified by our evaluation of the evidence.

It has been pointed out (Royall, 1997) that policy-relevant evidence evaluation involves at least two very different questions, which often are confused. In the EMF context, these two questions are: (1) Does the evidence developed specifically about EMFs support the "hazard" hypothesis more than the "no-hazard" hypothesis?; and (2) How probable is it that EMFs are a hazard? Royall makes the case that the first question can be answered by inspecting the statistical relative likelihood or Bayes Factor to see if it is greater than 1.0 and, if so, by how much. Others (Lindley, 2000) would argue that non-experimental examples require consideration of biases and confounding and not a mere consideration of the relative likelihood of non-chance vs. chance. So, when the reviewers talk heuristically about the strength of the evidence as a question separate from Question 2, below, they mean their overall assessment of the relative likelihood of the evidence after considering bias, confounding, and chance. The reviewers use this construction even though it would not be easy to quantify and they do not attempt to do so as a separate step.

The second question requires considering both the prior and the strength of evidence. As noted, if the prior is very small, the usual run-of-the-mill strength of evidence will not be sufficient to convince us that the posterior probability of an EMF hazard is large.

Because of the difficulty of translating complex evidence into numbers, the reviewers only use the ideas behind the formula as a way of explaining how certain or uncertain they were to begin with and to explain the basis for the weights they gave a particular stream of evidence in order to update our degree of certainty. The Bayesian perspective used by the California reviewers recognizes that a reassuring pattern of evidence from a stream of evidence that often misses a harmful effect does not allay one's suspicion much, even though an alarming pattern of evidence from that same stream of evidence might increase suspicion a lot. Going back to the hunter-gatherer example: if birds sometimes survive eating fruits that are lethal to humans, then reassuring evidence from bird experiments would not allay suspicion as much as the death of the birds after eating the fruit would increase our suspicion. In the terminology of probability, the relative likelihood conveyed by a positive or negative result depends on the false-positive rate and false-negative rate characteristic of that stream of evidence. The mathematical basis for this insight is discussed in the REGs (www.dhs.ca.gov/ehib/emf). It resulted in realizing that any stream of evidence, judged by the extent to which it usually produced false-positive and/or false-negative results, could be classified into four possible types: 1) capable of strengthening OR weakening one's certainty, 2) predominantly capable of strengthening certainty (like the bird feeding example given above), 3) predominantly capable of weakening certainty and, 4) uninformative, neither capable of strengthening nor weakening one's confidence. While this structured discussion helped organize the reviewers' judgments, it did not involve a mathematical combination of weights as would be the case in a quantitative Bayes evaluation. It should be noted that the Hill's attributes are like the bird feeding example. If they are present they strengthen confidence, but if they are absent, confidence falls only a little.

In the "Qualitative Bayes Approach," the DHS reviewers elicited their own expert judgment about the *a priori* (initial) probability of hazard after a special training session on how to avoid common errors of probabilistic estimation. It was important to be explicit about the prior probability because some physicists were arguing on the basis of physical theory applied to simplified biological models of the cell, that any biological effect from residential EMFs was impossible and thus had a vanishingly small initial credibility. This meant that they would require extraordinarily strong specific evidence to change their initial impression. Previous risk assessments have not explicitly considered this issue.

The discussion then turns to the patterns of specific EMF evidence in biophysical, mechanistic, animal pathology, and epidemiological streams of evidence. Obviously, if all four streams of evidence pointed toward or away from an EMF effect, the reviewers' job would be easy. But what if some streams of evidence are supportive and some are not? What weight should be given each stream of evidence? It was in the effort to address this problem that discussions of the inherent proclivity to give false positive and negative results came into play. This discussion was guided by a series of pre-agreed-upon questions described in the table above. The discussion included pro, con, and summary arguments. An example of such arguments are presented in the next table:

TABLE 1.2 EXAMPLE OF PRO, CON, AND SUMMARY ARGUMENT

CHANCE		
AGAINST CAUSALITY	**FOR CAUSALITY**	**COMMENT AND SUMMARY**
(A1) Not all the associations (relative risks) are above 1.00 or statistically significant.	(F1) The narrow confidence limits in the meta- analytic summaries and the low likelihood of this pattern of evidence by chance leans away from chance as an explanation.	(C1) A non-chance explanation must be sought.

Considering this kind of structured discussion helped organize the reviewers' judgments, after they weighed all the information in the usual way, although it did not involve a mathematical combination of weights as would be the case in a quantitative Bayes evaluation. After consideration of this carefully structured discussion of the evidence (considering how much more—or less—likely the pattern of evidence would be if the risk hypothesis were true compared to the likelihood of that evidence if EMFs were safe), the reviewers expressed an expert judgment on the posterior probability of a causal relationship.

1.4 QUALITATIVE BAYES RISK EVALUATION COMPARED TO TRADITIONAL AND QUANTITATIVE BAYES RISK EVALUATIONS

The traditional risk assessment has a section in which a judgment is given as to whether the agent being evaluated is capable of causing cancer or some other adverse health effect. This is called the "hazard identification." The typical presentation is heavy in describing the relevant evidence and rather light in explaining the rationale for the conclusion. Often the weight, given mechanistic, animal pathology, and epidemiological streams of evidence, depends on a review panel's interpretation of adjectives which best describe the pattern of evidence. For example is the pattern of evidence "sufficient" or should it be called "limited"? Can confounding and bias be "reasonably" discounted? Then there are pre-agreed-upon rules for combining the streams of evidence. Limited animal evidence plus limited epidemiological evidence results in one rank, sufficient animal evidence plus limited epidemiological evidence leads to another rank, and so forth. The combinatorial rules are straightforward, but the rationale for deciding that a stream of evidence is "limited" is not clearly defined and is subjective.

A completely quantitative Bayesian approach of the sort proposed by McColl et al. (McColl et al., 1996), or by Lindley (Lindley, 2000), would require assigning many quantitative parameters to a complex Bayesian Net model which would mathematically combine the subjectively assigned parameters to produce a posterior degree of certainty of causality. To the reviewers' knowledge, this kind of model has never been applied to any environmental agent and the DHS reviewers' stakeholders urged them to opt for transparency rather than mathematical elegance.

In response to the third draft, the Electric Power Research Institute contracted with Professor Sander Greenland in late 2001 to prepare a quantitative Bayesian model based on the epidemiological evidence for childhood leukemia. Since his will be the only extant quantitative Bayesian analysis, the reviewers contrast its proposed approach to their own. His model will provide a posterior dose-response curve based on a prior dose-response curve, the pooled epidemiological data, and prior estimates of selection bias and non-differential measurement bias. The all-important biophysical, mechanistic, and animal pathology streams of evidence will not be part of Greenland's model, although they could influence the prior dose-response curve in a subjective way. Calculations from Greenland's model would allow one to provide a probability that the posterior slope of the dose-response curve is not flat, that is, that there is some causal effect.

The following table compares the Qualitative Bayes evaluation to the traditional and to Greenland's Quantitative Bayes approach to risk evaluation as to a number of characteristics.

TABLE 1.3 COMPARISON OF USUAL RISK ASSESSMENT METHOD TO QUALITATIVE AND QUANTITATIVE BAYES METHODS

CHARACTERISTIC	USUAL METHOD	QUAL. BAYES	QUANT. BAYES
Evaluates all streams of evidence?	Sometimes	Yes	Focuses on epidemiology, other streams influence prior
Elicits prior probability?	No	Yes	Prior dose-response curve
Compares likelihood of each element of the evidence under the hazard and non-hazard hypotheses?	No	Qualitatively	Quantitatively with many of the parameters subjectively elicited
Pro, con, and summary arguments to make rationale transparent?	No, most risk assessments are skimpy in justifying hazard categories assigned	Yes	Not unless a supplementary document were to accompany the model
Combines relative likelihoods mathematically to derive posterior?	No	No	Yes, but non-epidemiological evidence is folded into the prior subjectively
Elicits an expert posterior probability after considering all elements of the evidence?	No	Yes	No
Displays judgments of various judges separately?	Usually strives for semblance of consensus	Yes	Technically possible for different experts to elicit their own parameters
Frames intermediate degrees of certainty as "not a proven hazard?"	Often	No, reveals posterior probability	No, reveals posterior probability

Both the Qualitative Bayes and the Quantitative Bayes evaluations can provide a posterior degree of certainty that the epidemiological associations are causal, which, if in the range from 10 to 90 out of 100, will not seem trivial to the general public and will stimulate policy discussions. The statements, “possible,” “there is no proven hazard,” or “there is no consistent evidence,” often used for this range of degrees of confidence, will not stimulate such discussions. Thus, both the Qualitative Bayes and Quantitative Bayes methods pose risk communication “problems” for those who believe that society should not begin policy discussions until most scientists are virtually certain that a hazard exists. The traditional hazard identifications would pose the same “problem” if they routinely used more nuanced categories of hazard assessment that distinguished between, say, a certainty level of 11/100 and one of 89/100. As now framed they pose a risk communication “problem” for those who believe that policy discussions should begin even before a hazard is firmly established.

Compared to traditional qualitative evaluations, the Qualitative Bayesian approach makes the evaluation more transparent, but it still accommodates different opinions. The DHS reviewers have no doubt that critics of their conclusions could use the Qualitative Bayes format to make their points. Some of the physicists who believe that they have a theory to prove that no residential EMF effect is possible would use priors so low that their posterior degrees of certainty would be low as well; the toxicologists who believe reassuring animal tests prove that EMFs are safe would make a case that the animal study results decrease their degree of certainty of a hazard to a level below their initial degree of certainty. In a contentious area such as EMFs, the reviewers doubt very much that any of the three styles of risk evaluation discussed in the table would force a consensus among subject matter experts who weigh and interpret the several streams of evidence differently. Even in the Quantitative Bayes model experts will use different priors and will elicit different subjective relative likelihood parameters for items like bias and confounding, for which there is no direct evidence. In the traditional method, experts will disagree on whether a stream of evidence warrants the adjective “limited” or “sufficient,” and in the Qualitative Bayes approach experts will disagree on “how much more likely” the pattern of evidence is under the causal and non-causal hypotheses. But the reasons for these different judgments will be more transparent in the Qualitative Bayes style of risk evaluation and we believe that this is desirable in controversial areas.

1.5 WHO DID THE EVALUATION AND WHAT FORM DID THE CONCLUSIONS TAKE?

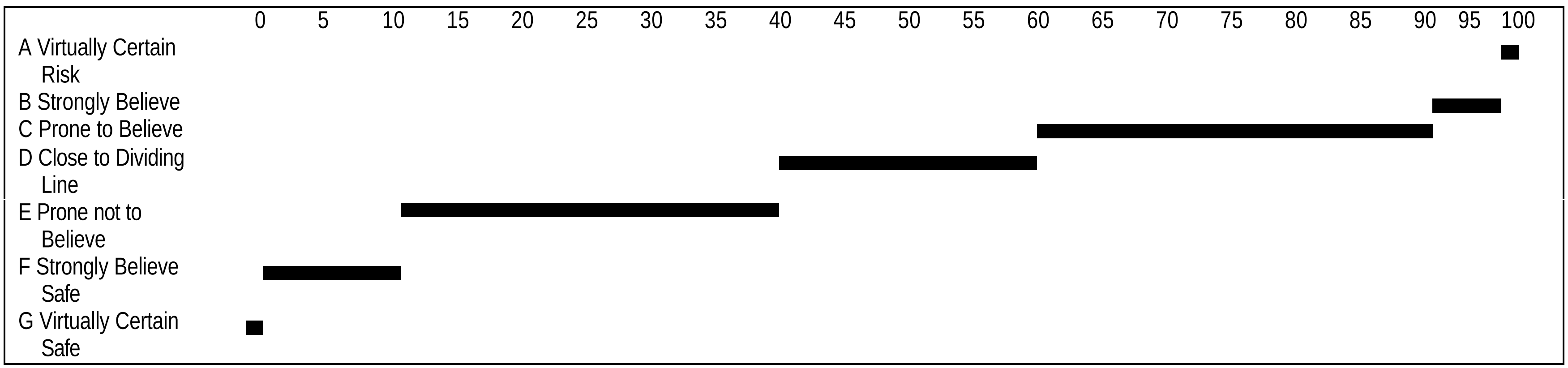
On behalf of the California Public Utilities Commission (CPUC), three scientists who work for the DHS were asked to review the studies about possible health problems from electric and magnetic fields (EMFs) from power lines, wiring in buildings, some jobs, and appliances. The CPUC request for review did not include radio frequency EMFs from cell phones and radio towers. Reviewer 1, Vincent DelPizzo, Ph.D., is a physicist and epidemiologist; Reviewer 2, Raymond Richard Neutra, M.D., Dr.P.H., is a physician epidemiologist; and Reviewer 3, Geraldine Lee, Ph.D., is an epidemiologist with training in genetics. All three have published original research in the EMF area and have followed the field for many years. To integrate and extend their body of knowledge, the EMF Program contracted with specialists in biophysics, statistics, and animal experimentation to prepare a background in critical literature review in their respective fields to make sure that the literature review was up to date through June 2000 (P Gailey Ph.D., G Sherman Ph.D., W Rogers Ph.D., and A Martin Ph.D.). The first three were involved with the writing of the 1998 NIEHS report. Furthermore, for each chapter of the review, another DHS epidemiologist or toxicologist was asked to read the original literature and consulted extensively with whichever of the three core reviewers was writing that chapter. This ensured that the writer based his/her evaluation on an understanding of the evidence that was as objective and consistent as possible. All three reviewers worked for the EMF program for at least five years and to some extent they influenced each other’s thinking through their constant interaction and the review of each other’s chapters. All three did their reviews according to the guidelines that had been developed earlier and approved by the program’s Science Advisory Panel (SAP). The Guidelines specified that the conclusions about any hazard should be done using two systems. The first was developed by IARC and has been used by NIEHS. It rates an agent as a "definite," "probable," "possible," or 'not a" carcinogen, or specifies that the evidence is “inadequate” to rate the agent. In addition, the California Guidelines specified that in order to accommodate the probability-based computer models of the program’s policy projects each of the DHS reviewers would individually assign a number between 0 and 100 to denote their degree of certainty that epidemiological associations between EMFs and certain diseases were causal in nature. The Guidelines, which were modified with advice from public comment and the SAP and the DHS reviewers, attached pre-agreed-upon English language phrases to various ranges of this degree of certainty. These are presented below in Table 1.4.

If all three judges had best judgments above 50 out of 100, but that fell in different categories in Table 1.4 judges were said to be "inclined to believe" that EMFs increased the risk of that disease to some degree.

If they found themselves in different categories below that point, they were said to be “inclined not to believe" that EMFs increased the risk of that disease to any degree.

TABLE 1.4 EVERYDAY ENGLISH PHRASES TO DESCRIBE DEGREES OF CERTAINTY OF CAUSALITY (GRAPH ILLUSTRATES THE RANGE OF CERTAINTY NUMBERS TO WHICH THE PHRASES PERTAIN)

ARE THE HIGHEST EMFS AT HOME OR AT WORK SAFE, OR DO HIGH EMFS INCREASE THE RISK OF .... TO A DEGREE DETECTABLE BY EPIDEMIOLOGY?	DEGREE OF CERTAINTY ON A SCALE OF 1 TO 100
Virtually certain that they increase the risk to some degree	>99.5
Strongly believe that they increase the risk to some degree	90 to 99.5
Prone to believe that they increase the risk to some degree	60 to 90
Close to the dividing line between believing or not believing that EMFs increase the risk to some degree	40 to 60
Prone to believe that they do not increase the risk to any degree	10 to 40
Strongly believe that they do not increase the risk to any degree	0.5 to 10
Virtually certain that they do not increase the risk to any degree	< 0.5



1.6 DOES PHYSICAL THEORY MAKE AN EVALUATION UNNECESSARY?

A number of scientists (mainly physicists) have expressed the opinion that the hypothesis that environmental EMFs are hazardous is intrinsically implausible and, therefore, all empirical evidence supporting it must be regarded as artifactual. In the Bayesian language, the prior—if not truly zero—is so vanishingly small that any realistic value of the relative likelihood conveyed by the evidence will inevitably fail to produce large posterior odds. Therefore, in their opinion, society should stop paying attention to this issue altogether. The DHS reviewers do not agree with this position. Because they did not find that the theoretical arguments were strong enough to dismiss the hypothesis out of hand, they proceeded with the evaluation of the evidence according to the REGs. Nonetheless, the reviewers do consider this and other relevant arguments for large and small prior degrees of confidence that EMFs might cause disease.